Comment on essd-2021-329
Response to Referee #1

Referee comment on "Chlorophyll-*a* growth rates and related environmental variables in global temperate and cold-temperate lakes" by Hannah Adams et al., Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2021-329-RC1, 2022

Review of Adams et al., Chlorophyll-a growth rates and related environmental variables in global temperate and cold-temperate lakes. ESSD-2021-0329

General:

This well-written data paper collates and integrates several commonly used open access water quality data sets (time series), including those from Fisheries and Oceans Canada (now IISD) Experimental Lakes Area, the US EPA, and the US Northern Lakes LTER Program, as well as substantial datasets from Alberta (Canada), England (UK), and Sweden. The authors summarize some, but not all, of the main features of the data, and provide some illustrative relationships among in situ and external environmental parameters designed to stimulate further analysis. One key feature of the data is the calculation of daily Chl a 'growth rates" (really, rate of change) that the authors use as a surrogate for phytoplankton population change. By identifying periods of growth > 0.5 ug Chl a/L/day, the authors examine how patterns of growth vary as a function of lake trophic status, irradiance, and limnological parameters both before and during the windows of growth. This analysis is both the strength and weakness of the paper, as the protocol does not appear to be used a prior peer-reviewed methods or application paper and is insufficiently validated in the current submission to allow its reliability or utility to be determined. Basically, the paper appears half-way between a simple data paper and a methods paper, and so is not really a good fit to either category at the moment. Key comments are outlined below, arranged according to the line number of the paper.

We thank the reviewer and appreciate their suggestions and comments for improving our manuscript; all their suggestions have been considered in the revision of the manuscript. Below, we provide the answers to the comments and questions raised by the reviewer and all the modifications that have been incorporated in the revised version of the article.

Details:
Terminology, l 49 and elsewhere. I think the authors would be better served to use less jargon and be more precise in their description of regulatory processes. Phrases such as "Bottom-up" and 'top down" appear derived from older food-web literature, and are used in a non-standard manner in the current paper. Here, 'bottom-up' appear to mean physical and chemical controls (physico-chemical), whereas 'top-down' seem to refer to grazing by zooplankton. I suggest they use these more precise description, as the current terms lack context when only one trophic level (phytoplankton) is actually described (as Chl a).

We agree with the reviewer. We removed unnecessary jargon and used the descriptions suggested by the reviewer (e.g., zooplankton grazing rather than top-down control). For example, on lines 80-83, we state "Intra-annual fluctuations in lake chlorophyll-a concentration result from the interactions of multiple variables and processes including grazing by zooplankton, competition between algal species with different growth strategies and chlorophyll-a contents, and changes in temperature, light, and nutrient availability."

Similarly, this appears to be a paper about the rate of change of Chl a standing stock/crop, not production (an amount) or productivity (a rate) per se. Its ok to note that changes in Chl are used to infer these other processes, but try and focus this paper on Chl to avoid over-extrapolation of the analyses (see below). This gets to be an issue when discussing the 'two phases' of change in Chl, separated by what is commonly known as
the clearwater phase (CWP). The latter occurs in productive (but rarely oligotrophic) lakes and arises when thermal stratification coincides with development of populations of large- bodies zooplankton (often *Daphnia*) to greatly reduce spring biomass maxima due to sedimentation (mainly diatoms) and herbivory (usually diatoms, flagellates). This decline in Chl does not necessarily correspond to declines in the rate of production (productivity), as available nutrients are usually elevated (because of recycling of biomass via
homeostatic herbivores). Jim Elser and Bob Sterner have done a lot of work on this (see "ecological stoichiometry"). The main point is that rates of change in Chl do no necessarily correspond to changes in primary productivity, particularly through a typical annual phenology.

We fully agree that the rate of change of the chlorophyll-a concentration is not solely related to algal productivity. To avoid any ambiguity, we modified the terminology used in the manuscript. The term "growth" is no longer used, and the following clear definitions are now given in the text. Specifically, we have renamed:

- *"Growth rate" as "rate of chlorophyll-a increase" (RCI), defined on line 115-116*
- *"Growth window" as "period of chlorophyll-a increase" (PCI), defined on line 107*
- *"Specific growth rate" as "normalized rate of change in chlorophyll-a concentration" (NRCC), defined on lines 182-183*

Renaming the "specific growth rate" to "normalized rate of change in chlorophyll-a concentration" (NRCC) avoids the assumption that the rate of increase in chlorophyll-a concentration must be strictly proportional to the increase in algal biomass concentration.

We now avoid any unwarranted interpretation and provide cautionary statements where needed. We have reworded our explanation about what the calculated rate of chlorophyll-a increase (RCI) represents and what the limitations of this metric are in lines 263-276. We have also highlighted in the text that the normalized rate, NRCC, is a relative rate which facilitates the comparison between lakes of different trophic status, whereas the RCI will vary systematically between lakes with different standing stocks of chlorophyll-a (lines 263-276). We have calculated the NRCC to use as the threshold for defining the start date of the PCIs and reported it as a variable in the dataset.

Introduction and elsewhere – literature cited. I suggest that the authors carefully review their citations, as I noted several instances when inappropriate references (e.g., marine, satellite) were used to describe or infer in-lake processes.

Agreed. We have replaced marine and satellite references with ones relevant to lake processes or, where appropriate, we added an acknowledgement of the source context. For example, see lines 102-103: "… these studies analyzed chlorophyll concentrations derived from satellite observations rather than measured in situ."

85-92. The presumption that bottom-up controls predominate in spring, whereas top down are most important in summer is not valid. As noted above (and in many many other papers), grazing control of phytoplankton is strongest during the CWP, and diminishes in summer when slow growing colonial cyanobacteria and algae escape grazer control. (see Carpenter and KItchell Trophic Cascade book etc). Without directly measures of grazing (or actual limitation by physico-chemical processes), the authors are setting up an unnecessary (and likely incorrect) framework. I think they might be better served by using the CWP research as a rationale for expecting 1 or 2 phases of phytoplankton biomass change ('growth windows') as they are well studied and explain the patterns seen in the analysis of Chl. The Plankton Ecology Group (PEG) model may be useful.

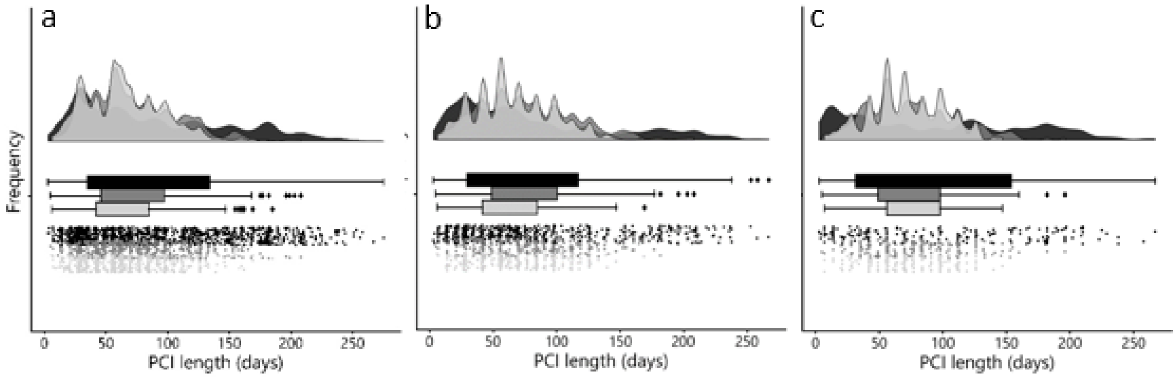
We agree with this comment and now refer to general concepts in lake productivity into the introduction and refrain from setting up a specific interpretative framework. These concepts help provide a broad context to potential users of the database. Note, however, that we now avoid unnecessary speculative (and controversial) interpretations of the patterns and trends in the data. As such, our revised manuscript is foremost a data paper on PCIs and RCIs that can be used by the research community.

Data and methods. Perhaps I missed it, but I think the paper needs a table which explicitly lays out the data sources without requiring the readers to go to the actual database. As part of this, there should be a good array of summary data (dates samples, resolution, parameters, lakes, etc). While I recognized most of the sources of data, I did not find direct attribution of the data to individual investigators within the paper, which I think is inappropriate.

We fully agree and have added a summary table and data author citations found in the published metadata to the supplementary materials and references section of the manuscript.

As part of this, I think the authors need to justify the re-publication of already open- source data. I suspect the rationale is that there is a new analysis of Chl a dynamics, but that in itself creates the problem that the method of Chl analysis has not be validated previously (mainly section 2.2). The simplest solution is to hold back publication of this data paper until the method is reviewed and evaluated. Alternately, the authors need to demonstrate more robustly that the method they use is reasonably artifact free (e.g., the statement that growth periods range 2 to 260 days is highly suspect – see below) and very likely dependent on time series resolution.

We are presenting a dataset that includes data that has been reformatted and augmented with the estimations of RCI, PCI, and NRCC values. In doing so, we follow common practice to summarize data and calculate additional metrics derived from those data. Our derived dataset is in compliance with the licensing for every original source used.



We agree that the range of the PCI length may be influenced by variable sampling resolution. To help users of the data navigate this, we now include the mean time between sampling for the *data series in our dataset. Thus, the user will be able to filter the database for a desired temporal resolution, for example by selecting only lakes that are sampled monthly, which is a common sampling frequency for many monitoring programs. Furthermore, we have added the extra figure shown above in the supplementary material. The figure shows that the sampling frequency does not have a strong influence on the PCI length distribution. (Note: the figure compares the distribution of PCI lengths for a) the full dataset, b) the dataset filtered for a maximum of 31 days between samples, and c) the dataset filtered for a maximum of 14 days between samples. While the frequency distribution does change somewhat, the range of PCI length is maintained even when selecting for the data with more frequent sampling.)*

Data compilation – I think the authors need to provide much more information on how the data were harmonized. I know from research in the US LAGOs program (Soranno et al), this is a massive part of the compilation process, taking years in the case of large datasets. Just saying that chemistry was measured (l. 125-130) seems insufficient to me. How did techniques differ, does that affect the findings (or not), are old procedures replaced by new ones, etc.

We expanded the methods section to provide more details on the harmonization of the dataset; see Section 2.1.1. We refer readers to the augmented source summary table, so that they can easily access the metadata related to the techniques used to generate the source data.

Similarly, lake selection needs to be better justified. First, it makes absolutely no sense to include two low latitude lakes, famous or not, in a high0latitude study (remove them; delete l. 130-135). Second, the authors do not appear to recognize that climate systems (Gulf Steam, NAO, etc.) affect latitudinal gradients, producing much different local conditions in the UK/Scandinavia/EU than in North America (mainly continental Canada). I understand this is not a full analytical paper, however, the use of latitude is uncritical acceptance and can confuse patterns presented in this paper.

We have removed these two lakes from the dataset.

We agree that latitude alone does not determine local climate conditions. Therefore, we now also identify the actual climate zone of each lake in the revised dataset. However, we still provide the lakes' latitudes in our dataset because they affect the photoperiod.

Fig. 2 – I like the workflow but is it also possible to see how the number of sites/data density changes through the process? (how much data is lost at each step). Or that could be in an appendix figure.

We have updated figure 2 to incorporate the changing data density throughout the workflow.

141-150. Please provide more information on data manipulation. This is too vague/unclear to allow replication.

We have reformatted and expanded the methods section to provide more information on the data manipulation process; in particular, Sections 2.1.1 and 2.2. Note that most of the added detail is placed in the supplementary information, which we now refer the reader to within the text (lines 193 and 215-216). Please, also note all the code used to clean, format, and generate the PCI dataset can be freely accessed in a public GitHub repository and on Zenodo.

153-155. I think the authors need to provide more summary data on the time series themselves. A statistical overview of the resolution seems particularly important, as the vast majority of sites would be sampled at weekly or longer intervals, so would be expected to have substantial restrictions on the detection of growth window onset and duration. Monthly resolution could, arguably, make the growth window meaningless.

As suggested by the reviewer, we have expanded the data source summary table to include information about the sampling resolution of each data series. We have also expanded the information about the derived data (i.e., RCI, PCI, NRCC) by including a column containing the sampling resolution of the corresponding source data. We have included a summary figure (Fig 4b) in the manuscript illustrating the distribution of sampling resolution within the compiled data sources to be transparent about the variability in source data sampling resolution.

SSR. The use of these data need much better justification. First, as shown from the map in Fig. 1 – the vast majority of lakes are not located very close to SSR stations. Second, cloud cover would be expected to greatly influence the receipt of solar irradiance, but would not be recorded well for individual lakes. I *like* the attempt to use SSR data to explain variation in growth windows, but find the approach was largely unsuccessful – likely because the data were inappropriate. I think the authors need to better justify the use of SSR or drop the analysis.

We agree with the reviewer that the distances between the lakes and the SSR stations is a major limitation. However, SSR is an essential variable when considering trends in lake productivity. Our dataset on SSR is the result of our best efforts to collate the data that are available. By providing the distance between a lake and the corresponding SSR station, the user can make up their own mind as to whether meaningful inferences can be made or not. The user can also use a cut-off distance to filter the dataset. We hope that our dataset will call attention to the need for more direct over-lake SSR measurements, particularly in view of the mounting evidence that secular changes in SSR are modulating photosynthetic rates in terrestrial ecosystems. In the revised manuscript we have added a figure showing the frequency distribution of lake-SSR station distances, and state that "Users are therefore advised to consider this limitation [of distance] when making use of the SSR values in our dataset" (lines 314-315). We also added a sentence in the text to highlight the potential impact of cloud cover and aerosol variability on the representativeness of the SSR measurements: "…in a significant number of cases, the actual mean SSR during a PCI may differ from the in situ mean SSR reported here, due to differences in cloud cover and levels of atmospheric aerosols (among other factors)." (lines 311-313).

2.1.3 – the summary of lake characteristics is pretty limited. This would seem to be important if the data are to be used in other analyses (as inferred).

Information on the lake characteristics can be found in the summary metadata files accompanying our dataset. We have added a clarifying statement on line 183 directing readers towards these summary files so they can access this information: "The reader is referred to the "lake summary" file in the supplementary information for details on the lake characteristics."

Section 2.2. Detection growth windows. The paper hinges on readers accepting 0.05 ug Chl /L/d as a critical threshold for signifying 'growth'. Its use because it was the median value in lakes

with a CWP (productive; 2 growth phases) is one possible justification; however, the use of the same threshold in oligotrophic lakes seems inappropriate as Chl values may be up to two orders of magnitude lower than in eutrophic systems. Basically, use of a single static metric over all lake types seems unjustified.
One possibility would be to do a sensitivity analysis. The selection of a single threshold immediately raises the question of how the data patterns would change in a different threshold were used. Normally (in a methods paper), the authors would conduct some form of sensitivity analysis to demonstrate that the findings were (not) robust to the precise value used in the study. (but again, the authors need to decide whether this Is a data paper or a full analytical report).

We appreciate the thorough comments and suggestions for selecting a threshold value for the start of the growth window (which we have now renamed as "period of chlorophyll-a increase", PCI). To address this concern, we have first replaced the absolute rate of chlorophyll-a increase RCI as the threshold metric with the normalized rate of change in chlorophyll-a concentration (NRCC). We highlight in lines 263-276 that, in contrast to RCI, NRCC is a relative rate, which can be compared between lakes across a range of standing stock of chlorophyll-a and trophic classes.

Second, we have now included in our supplementary material the results of our sensitivity analysis of the derived PCI and RCI distributions to the imposed threshold NRCC value that is used to define the start of the PCI. A full explanation of the calculation of the normalized rate and the sensitivity analysis is included as online supplementary material and referred to in the text on lines 190-192. Briefly, we have performed a sensitivity analysis for NRCC threshold values ranging from 0 to 1.2 d^{-1}. Results from a Kruskal-Walis test showed that the PCI start day and PCI length change significantly when the threshold NRCI approaches zero. Based on the results of the sensitivity analysis, we have selected a threshold value of 0.4 d^{-1}. This relatively high value reduces the risk of erroneously including the lead-up time to the PCI. Hence, the calculated rate RCI is more likely to be a measure of the exponential increase in algal biomass.

Data set. As noted about, some summary characteristics of the lakes (morphometry), Chl time series (sampling resolution, etc) and chemistry would be useful to interpret the patterns seen in l 215-224.

As stated earlier, we direct the readers on lines 179-180 to the lake summary files in the supplementary information where they can find this information and related data (including the sampling resolution): "The reader is referred to the "lake summary" file in the supplementary information for details on the lake characteristics."

220-221 and elsewhere. Try to be less declarative about the reason for unmeasured patterns. In North America, latitude is correlated inversely with human populations and activity. To an extent, this is true in the European continent.

Agreed. We have removed the sentence and reworded other declarative statements throughout the text.

L 229-231 again cause of patterns two speculative (and see comments about CWP above)

Agreed. We have removed the speculative statement.

253-263. This paragraph is overly speculative, as it is based on marine gradients, not those of lakes. See work of John Smol for patterns in Northern Canada.

Agreed. We replaced references to marine gradients with the suggested references to the work of John Smol.

282-285. Again, this is over-extrapolated (from a fossil study that does not prove mechanism). More importantly, the analysis of SSR is vague or unreproducible. "Sensitivity" to SSR is never defined, and there is no obvious relationship in panels of Fig. 9. This whole section in unconvincing because there is no clear relationship among variables, and because the SSR sites are not particularly close to the study lakes.

Agreed. We removed the reference to the McGlue et al study from the paper. We have also replaced the interpretative SSR correlation analysis with Section 4.3, a description of trends seen in the SSR data within our dataset.

Key findings. In my opinion, too many of the findings infer mechanisms which were not proved rigorously in the paper. It's fine to review the main patterns, but it's inappropriate to infer causal mechanisms from the sorts of analyses presented. Again, I think the authors are trying to move this from a data paper to a methods or interpretive paper. While this may make the paper more scientifically interesting, I don't find it appropriate for the format of a data paper.

We have removed this section from the text in favour of focusing on presenting the trends as examples of observations that can be extracted from the dataset (without going into speculative interpretations).